

**Left by a Trusted Physician and Colleague:  
Impacts of Primary Care Physician Exits in  
a Group Practice System**

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# Imprint

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# Left by a Trusted Physician and Colleague: Impacts of Primary Care Physician Exits in a Group Practice System

Lina Maria Ellegård\*, Gustav Kjellsson†, Daniel Monsees‡

## Abstract

Disruptions of the patient-general practitioner (GP) relationship due to GP exits affect patient outcomes in various ways. The literature suggests that the loss of a trusted professional can have negative effects, but also points at the benefits of being examined by a new GP. We study the impact of GP exits in the Swedish primary care setting, in which care is provided in group practices and even those patients who have a main GP usually see different GPs. Since the practice is responsible for organizing a replacement GP after an exit, we expect the transition between GPs to be relatively smooth for patients. Using two different control groups we are able to separate the effects of being affected by the loss of ones main GP from the reduction of a practices' workforce. Our results show that GP exits have a negligible effect on patients health care utilization in the Swedish setting. We do observe some shifts in health care utilization patterns, however, this is not driven by the loss of the main GP but rather practice level disruptions. These findings suggest that in organizational settings such as in Sweden - where continuity of care is maintained at practice rather than GP level - patient outcomes are less dependent on individual GPs. Importantly, this conclusion holds even for patients with high baseline continuity of care.

## 1 Introduction

A longstanding relationship between the patient and the general practitioner (GP) is widely held as a core component of high-quality primary care. Against this background, the aging of the GP workforce in many countries is concerning, as many patients will soon face the loss of their GP due to retirement. In 2022, 35% of doctors in the European Union were over 55 years old. In Sweden, this figure was 26%, while reaching 42% in the US (OECD, 2024; GlobalData Plc, 2021). At the same time, many health care systems face persistent challenges in recruiting and retaining GPs (Shen et al., 2020; Kroezen et al., 2023; Lee and Kontopantelis, 2024; Wang et al., 2025; Ellegård et al., 2025). Turnover rates have increased globally, reflecting a broader trend in which GPs,

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similar to other professional groups, are less inclined to remain at the same workplace for long periods (Parisi et al., 2021; Bond et al., 2023).

GPs leaving a practice or exiting the workforce inevitably disrupt established doctor–patient relationships and reduce continuity of care. An extensive literature shows that low continuity of care is associated with adverse outcomes such as hospitalizations and mortality (Baker et al., 2020; Yu-Hsiang Kao et al., 2019). The correlational literature has recently been advanced by a growing event study-based strand examining the impact of GP exits in various settings.<sup>1</sup> These studies confirm that the GP-patient relationship is important, but also nuance the picture from the correlational literature on continuity of care as they find mixed effects on patients’ utilization of primary care and secondary care – including hospitalizations. An important insight from this literature is that exits may have beneficial effects. Specifically, exits can speed up the detection of cancer and chronic conditions, as the exiting GP increases effort before quitting (O’Halloran et al., 2021) or the patient gets examined by a new person following the exit (e.g. Kwok, 2024; Simonsen et al., 2021; Zhang, 2022).

Given the inevitability of GP exits, whether through retirement or change of practice, a key question is whether primary care can be organized in a way that makes exits less consequential. In an ideal world, GP exits would be neither positive nor negative for patients’ health, and would not reduce patients’ reliance on primary care as the first line of care. One approach would be to make patients less dependent on a specific relationship in the first place. This is possible in a group practice setting, which allows patients to visit different physicians at any point in time and where patients do not have to search for a new GP following an exit.

One example of such a setting is Sweden, where primary care is provided by large multi-professional group practices. Most of the environment remains intact following the exit of an individual GP, making the transition to a new GP close to a seamless process: patients remain registered in the practice after the exit and thus do not have to search for a new GP. Also, and importantly, Swedes commonly meet several different GPs in the practice, even if there is a specific GP that they see most often. Thus, being examined by a different physician than usual is a common experience, and nothing that signifies the period around the exit of one’s main GP. These aspects of Swedish primary care make it an interesting setting to further the literature on GP-patient relationships.

To be able to study disruptions of GP-patient relationships, we concentrate on patients with relatively many GP contacts over a 3-year horizon, allowing patients to build a relevant relationship with their main GP. Our study population met their most commonly seen GP on 70% of consultations in the years leading up to the exit, showing that they had high continuity as well as experience of seeing other GPs. We compare patients exposed to the exit of their GP to two control groups, one consisting of patients in other practices with no concurrent exit and one consisting of patients in the same practice, who never saw the exiting GP. Using these two control groups allows us to separate the impact of a GP exit on the practice level – affecting among other things the workload of remaining GPs – from the impact on the GP-patient relationship itself. Our results indicate only small changes in health care utilization after the exit, which are mainly due to the impact

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<sup>1</sup>Specifically Denmark (Simonsen et al., 2021), Norway (Vinjerui et al., 2024), Germany (Monsees and Westphal, 2025), Austria (Zocher, 2024), Switzerland (Bischof and Kaiser, 2021; Hjalmarsson et al., 2023), and different US settings (Medicare (Kwok, 2024; Zhang, 2022; Sabety, 2023), Medicaid (Staiger, 2022) and the Military Health Services (Schwab, 2025)).

on the practice level workload, and not to the breach in patient-GP relationships. The Swedish setting with group practices thus seems to be able to handle exits more smoothly than settings where primary care is provided by small-scale or solo practices.

We are not the first to suggest that group practices handle transitions more smoothly. Studies on the Medicare population (Sabety et al., 2021; Zhang, 2022), in which half of the patients see more than one GP annually, find that patients in group practices experience smaller reductions in primary care use and less substitution to secondary care. In a study set in the U.S. military health care system, Schwab (2025) points out that GP exits operate via two mechanisms in group practice settings: a disruption of the patient provider relationship and a reduction in the practice’s workforce. Viewing the second mechanism as a threat to identification, Schwab adapts his estimation strategy to exclude this pathway. We take a different perspective and regard this channel as important in its own right when analyzing the impact of GP exits. Indeed, our results suggest that the reduction in the workforce plays a larger role than the break in continuity. It is therefore essential to distinguish the impact of the disruption of the relationship from the impact of increased workload of remaining practice staff.

One part of the benefit of group practice is that it facilitates the assignment to a new GP following an exit. However, patients are also automatically assigned to new GPs in some settings where self-employed GP arrangements is the norm, e.g. Austria, Denmark and Norway. There is less evidence that GP exits reduce patients’ reliance on primary care in contexts with automatic assignments (Simonsen et al., 2021; Vinjerui et al., 2024; Zocher, 2024) than in context without automatic assignment (e.g., Bischof and Kaiser, 2021; Monsees and Westphal, 2025). This underlines the importance of a smooth transition between GPs. We also do not find that disruptions *per se* reduce the access to primary care. However, unlike the earlier studies, we do not find any evidence that exits affect the probability of getting a cancer diagnosis or treatment for a chronic condition. This suggests that the feature of regularly meeting several different GPs, which is common in Sweden but not in the other contexts with automatic assignment, may further contribute to making exits non-salient events.

Our study advances the literature on GP exits by examining a primary care system organized around large, multi-professional group practices where patients frequently see different GPs, reducing vulnerability to disruptions in individual physician-patient relationships. Beyond assessing the impact of such relationship breaks, we make a novel contribution by studying the effects of the disruption to the workforce caused by the loss of an experienced GP.

## 2 Institutional Setting

The responsibility for financing and organizing the Swedish publicly funded health system is decentralized to 21 regional health care authorities. In all regions, primary care is organized in multi-professional group practices employing GPs, nurses and other professionals such as physiotherapists and occupational therapists. Nurses are highly skilled and perform many tasks that would be done by GPs in other countries, including preventive activities, treatment of minor ailments, and management of chronic conditions (Janlöv et al., 2023).

Our study setting is Skåne, Sweden’s southernmost region with 1.4 million residents. Every resident is registered at a primary care center (PCC), which receives a monthly risk-adjusted capitation payment to cover the patient’s primary care needs. Patients may register at any PCC and practices were not allowed to close their lists during our study period. There are around 150 PCCs in the study region, half of which are privately owned, with typically 4-5 permanently employed salaried GPs. Vacancies are filled by doctors on fixed contracts. In addition, doctors in training, such as interns and residents, provide care in the PCCs. Residents specializing in general medicine are employed by a practice for around 5 years, while some of their training takes place outside primary care.

Patients usually attend the practice where they are registered and rarely switch PCCs (Anell et al., 2021). Around 10% of patients register at another PCC annually, mostly due to moves. Yet, even though the continuity of care is high at the PCC-level, the continuity with GPs is often low. This is partly due to a high turnover of physicians (Ellegård et al., 2025), and partly due to how practices organize care flows. When seeking care, patients speak to a triage nurse who decides if an appointment with a GP should be scheduled, how fast and with whom. Depending on the routines at the PCC, GPs may or may not be responsible for specific patients, and the triage nurse may or may not consider this when scheduling appointments. Even patients who regularly see a certain GP may be allocated to another GP if the main GP is not available, for instance if the issue is urgent. All GPs in the practice can access patients’ electronic medical records.

## 3 Data and sample

### 3.1 Data sources

We use two regional databases from Statistics Sweden to identify a study population and measure care use: a health care register covering all contacts with publicly funded healthcare providers in the region and a register of individuals enrolled at primary care centers. For each care contact in our data excerpt from the health care register, we have information on dates, up to eight diagnoses, contact types (visit/phone/letter), professional category (physician/other), care provider (practice and GP identifiers) and covers both primary and secondary care. These data cover 2005-2019, but before May 2009 the data do not have full coverage for privately operated providers.<sup>2</sup> Our data from the enrollment register covers 2009-2019. We also use data from Statistics Sweden’s registers to measure individual background characteristics such as age, gender, civil status, income, educational attainment and region of residence. The data sources are linked using a personal identification number.

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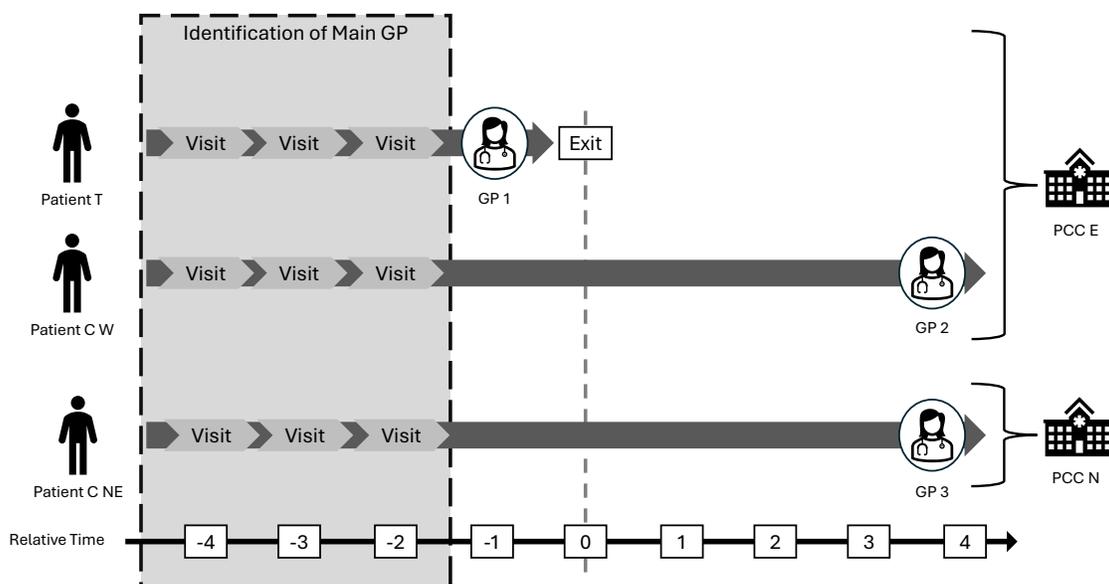
<sup>2</sup>The patient choice system described in section 2 was introduced in May 2009. Prior to the reform, the number of privately operated PCCs was much lower (Dietrichson et al., 2020). Contacts from these private PCCs were registered in a separate data system.

## 3.2 Sample selection

### 3.2.1 Definitions

We aim to study the impact of disruptions of GP-patient relationships. As patients normally encounter several GPs in the PCC where they are registered, we restrict the analysis to patients who have a "main GP", that is, a GP with whom they have a meaningful relationship both in terms of length and relative importance. The study population consist of patients affected by the exit of their main GP, plus two control groups. Figure 1 provides an overview over the identification of these three groups.

Figure 1: Sample Selection



First, we identify GPs who quit after working in the same PCC for at least 4 years, long enough to form relationships,<sup>3</sup> and who did not return to the same practice within 4 years. The end of such a GP-practice spell is denoted a "GP exit".<sup>4</sup> We do not distinguish between different reasons for the exits such as retirement or job switching. We consider exits in 2009-2015, henceforth *exit years*. Our analysis is centered around these exits; we use the running variable ( $t$ ) with  $t = 0$  as the year of the exit. Since our data cover the years 2005-2019, we can use a total observation window of  $\pm 4$  years for the entire study population, which is represented by the time scale *Relative Time* in Figure 1.<sup>5</sup>

<sup>3</sup>A GP-practice "work spell" is defined as a period during which the longest gap between days with consultations does not exceed one year. I.e., leaves of up to one year are allowed.

<sup>4</sup>Note that exits after shorter spells are not considered as "exits" in our terminology onwards.

<sup>5</sup>Because most private providers entered the market after the patient choice reform in May 2009 and the health

Second, we identify the subset of patients who had a main GP in a three-year period prior to a potential exit year. These are the years -4 to -2 in Figure 1. To qualify as a patient’s main GP, the patient must have had at least 1 visit with the GP in each of these years. Additionally, the GP must be the GP with whom the patient had the most visits in these three years. Note that it is possible – and highly likely – for patients to also see other GPs than their main GP throughout the study period.

Third, we identify the treatment group. Patients are considered *treated* in an exit year if their main GP exited that year. This is represented by *Patient T* in Figure 1, whose main GP is *GP 1*. Patient T had at least one visit with GP 1 in the years  $t \in [-4, -2]$  and GP 1 left in  $t = 0$ . Patients who were treated in multiple exit years are excluded from the analysis.

Lastly, we construct two control groups for each exit year. Both are based on the untreated individuals for which we could identify a main GP as defined above. However, their GP did not exit in  $t = 0$ , in fact, we require that their main GP stayed in the same practice during the observation window ( $t \in [-4, 4]$  around the placebo exit year). The first control group consists of patients registered at the practices with exiting GPs in the same year, henceforth *Within practice control*. This is represented by *Patient C W* in Figure 1, with main GP *GP 2*, in practice *PCC E*, where a GP exits in  $t = 0$ . The second control group includes patients registered at practices with no GP exit in the same year, henceforth *Non-exit practice control*. This is represented by *Patient C NE*, with main GP *GP 3*, in practice *PCC N*, where no GP exits in  $t = 0$ . To mitigate any direct influence of the exiting GPs, we exclude patients who had ever met the exiting GP from this control group.

A final restriction placed on both treated and control patients is that they must be living in the study region throughout the entire pre-period ( $t \in [-4, 4]$ ).<sup>6</sup>

### 3.2.2 Implications for identification

Three considerations are central for understanding our identification approach. First, the rationale for constructing two control groups is the following. In our group practice setting, exits not only cause a disruption of patient-GP relationships but may also affect the access to care if the PCCs struggle to replace the volume of care provided by exiting GPs by recruiting replacements. As we show in Appendix A.1, the exiting GPs constitute less than a tenth of the headcount but account for almost one fifth of the workload in their practices. Further, although PCCs recruit replacement GPs already in the exit year (as can be seen in Figure A.1d), the new GPs are much less productive than the exiting GPs. A reduction in access to care has therefore to be expected after the exit. The within practice control group includes patients who were also affected by this reduced access, but not by the disruption of the GP-patient relationship the treatment group experiences. Thus, with this control group we can isolate the impact of the breach in continuity from the impact of the higher workload for remaining GPs. On the other hand, the comparison of the treatment group

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care register data do not have full coverage from private providers prior the reform, exits up to 2013 are primarily concentrated to public PCCs. Therefore we explore heterogeneity across the years in Section 5.1.2.

<sup>6</sup>We define this as being registered as a resident in the region on December 31 according to the population register from Statistics Sweden.

with the non-exit practice control group captures both the impact of the breach in continuity and the reduced access to care.<sup>7</sup> Therefore, the non-exit practice control group identifies the overall effect of a GP exit on patients, similar to previous studies.

Second, restricting the study population to patients who visited the same GP at least once each year over a three-year period results in a sample with an unusually high level of primary care use. This is not only in relation to the general population<sup>8</sup> but also in relation to themselves later in the study period (Figure A.8). In other words, the sample selection criterion builds in a mean reversion pattern, a mechanical decline in GP visits in  $t - 1$  onwards. Though this may seem undesirable, recall that the purpose of the restriction is to identify patients with a meaningful relationship with a GP. In a setting where the population rarely visits GPs in general, and where it is unusual to see a specific GP regularly, the group fulfilling this criterion at any point in time will by necessity have unusual care use patterns. Also note that the treatment and control groups are subject to this selection in the same manner. We further account for the potential mean reversion effect by balancing the number of GP visits in the pre-period in Section 3.4.

Third, and related, our control groups only include never-treated individuals – we do not, explicitly or implicitly, use later-treated patients as a control. Most previous studies (e.g. Simonsen et al., 2021; Zocher, 2024) use later-treated patients as a comparison, arguing that the later-treated may be more similar to the earlier-treated. Adopting the same strategy would be problematic in our case, due to the mean reversion pattern: the later-treated by definition experience an increase in their primary care use in the part of the follow-up period corresponding to their own pre-period (i.e., "their"  $t \in [-4, -2]$ ).

### 3.3 Sample characteristics

In 2009-2015, 21,737 patients were affected by 323 GP exits, these patients represent our treatment group. They are compared to 60,395 patient in the control group with no concurrent GP exit (non-exit practice controls) and 28,278 patients in the same practice as the exiting GPs (within-practice controls).

Table A.1 shows descriptive statistics of health care utilization by treatment group for the pre-period ( $t \in [-4, -2]$ ). Overall, the study population had around 3 face-to-face visits and 1.5 to 2 phone contacts with a GP annually. They also visited a primary care nurse more than two times per year on average. These numbers suggest, as noted in Section 3.2, that the study population is relatively unhealthy. For comparison, the general population had approximately 1.5 GP visits per year. The study population is also older, with an average age of 64 (Table 1).

Table 1 shows descriptive statistics for key variables. Of especially high interest is the variable *UPCI Before Exit*, which shows the usual provider of care index, in this case the main GP's share of all GP visits in the three-year period used to define the study population. For the treatment

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<sup>7</sup>We also compared the two control groups with each other, however, the results can also be deduced from the main results and are therefore not included.

<sup>8</sup>In our study region, the average is 1 GP visit per year; one third of the population makes no visit in a given year.

group, 74% of visits were with their main GP. Given that patients made on average 3 visits annually in this three-year period, this is a reassuring finding as it suggests that their main GP did indeed provide a majority of visits, while also showing that patients encountered other GPs in the practice. The corresponding figures are similar for the control groups, even somewhat higher, e.g. 80% for the within practice control group.

To give an indication of the similarity of the treatment and controls groups, the tables also show the standardized mean differences (SMD) for each control group compared to the treatment group. SMDs above .10 are often viewed as “large”. The non-exit practice control group is quite similar to the treatment group. All SMDs except one, *Exit year 2015*, are below .10. The within-practice control group is also similar to the treatment group on most variables, except that they made .4 fewer GP visits annually during the three years we use to define the study population ( $t \in [-4, -2]$ ).

Table 1: Descriptives: Balancing Variables Pre Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
Exit Year 2009	0.154	0.361	0.150	0.357	0.012	0.150	0.357	0.010
Exit Year 2010	0.134	0.340	0.141	0.348	-0.021	0.144	0.351	-0.031
Exit Year 2011	0.182	0.386	0.118	0.322	0.180	0.157	0.364	0.066
Exit Year 2012	0.133	0.339	0.118	0.323	0.044	0.126	0.332	0.020
Exit Year 2013	0.117	0.322	0.109	0.312	0.026	0.140	0.347	-0.066
Exit Year 2014	0.152	0.359	0.135	0.342	0.047	0.127	0.333	0.072
Exit Year 2015	0.128	0.334	0.229	0.420	-0.265	0.156	0.363	-0.079
UPCI Before Exit	0.738	0.217	0.746	0.218	-0.036	0.799	0.190	-0.299
GP Visits in -2	3.027	2.326	2.945	2.309	0.036	2.594	1.935	0.202
GP Visits in -3	3.034	2.399	2.952	2.287	0.035	2.600	1.926	0.199
GP Visits in -4	3.026	2.272	2.913	2.225	0.050	2.607	1.912	0.200
Born in Sweden	0.817	0.387	0.838	0.369	-0.055	0.837	0.369	-0.053
Female	0.624	0.484	0.614	0.487	0.022	0.603	0.489	0.044
Age at Exit	64.182	16.027	64.202	16.035	-0.001	64.909	15.611	-0.046
Unmarried	0.166	0.372	0.159	0.366	0.019	0.155	0.362	0.029
Married	0.506	0.500	0.524	0.499	-0.037	0.526	0.499	-0.040
Divorced	0.181	0.385	0.172	0.377	0.023	0.168	0.374	0.035
Widowed	0.147	0.354	0.144	0.351	0.008	0.151	0.358	-0.011
Primary Education	0.346	0.476	0.340	0.474	0.014	0.352	0.477	-0.012
Secondary Education	0.421	0.494	0.426	0.494	-0.010	0.429	0.495	-0.016
Tertiary Education	0.233	0.423	0.234	0.424	-0.003	0.220	0.414	0.032
Log Disp. Income	11.905	0.731	11.937	0.773	-0.043	11.923	0.793	-0.024
HH Size	1.945	1.075	1.963	1.078	-0.016	1.922	1.045	0.021
Number of Observations	21737		60395			28278		

Note: SMD = Standardized Mean Difference compared to treatment group.

UPCI Before Exit = Share of contacts to most seen GP among all contacts before exit.

### 3.4 Entropy Balancing

The difference-in-difference approach employed in this paper normally does not require treatment and control group to have similar outcome levels. However, for reasons explained in Section 3.2, ensuring similar levels as baseline is important in our setting because the set of patients with a meaningful relationship with an exiting GP display a mean reversion pattern with respect to GP

visits. The pre-period differences in the level of GP visits shown in Table 1 are problematic since the control groups can be expected to decline less from their initially lower level for purely mechanical reasons. This is especially true for the within-practice controls. We therefore use a weighting approach, assigning more weight to control group members who are more similar to the treatment group, thereby ensuring similar pre-treatment levels of annual GP visits.

We use entropy balancing (EB), which is a data driven way to assign weights. The EB algorithm searches for weights that make selected statistical moments of the weighted control group as similar as possible to the moments of the treatment group (Hainmueller, 2012). We balance within exit years on the means of all other variables presented in Table 1.<sup>9</sup>

Table 2 shows the descriptive statistics after applying the entropy balancing weights for the two control groups. After weighting, there are no longer any large SMDs. Note that when using GP visits as an outcome, the balancing implies that the weighted control groups mechanically are equivalent to the treatment group on both levels and trends for  $t \in [-4, -2]$ .

Table 2: Descriptives: Balancing Variables Post Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
Exit Year 2009	0.154	0.361	0.154	0.361	0.000	0.154	0.361	0.000
Exit Year 2010	0.134	0.340	0.134	0.340	0.000	0.134	0.340	0.000
Exit Year 2011	0.182	0.386	0.182	0.386	0.000	0.182	0.386	0.000
Exit Year 2012	0.133	0.339	0.133	0.339	0.000	0.133	0.339	0.000
Exit Year 2013	0.117	0.322	0.117	0.322	-0.000	0.117	0.322	0.000
Exit Year 2014	0.152	0.359	0.152	0.359	-0.000	0.152	0.359	-0.000
Exit Year 2015	0.128	0.334	0.128	0.334	0.000	0.128	0.334	-0.000
UPCI Before Exit	0.738	0.217	0.738	0.222	-0.000	0.738	0.219	-0.000
GP Visits in -2	3.027	2.326	3.027	2.540	0.000	3.027	2.513	0.000
GP Visits in -3	3.034	2.399	3.034	2.449	0.000	3.034	2.460	0.000
GP Visits in -4	3.026	2.272	3.026	2.399	0.000	3.025	2.385	0.000
Born in Sweden	0.817	0.387	0.817	0.387	-0.000	0.817	0.387	-0.000
Female	0.624	0.484	0.624	0.484	0.000	0.624	0.484	0.000
Age at Exit	64.182	16.027	64.182	16.066	-0.000	64.183	16.071	-0.000
Unmarried	0.166	0.372	0.166	0.372	0.000	0.166	0.372	0.000
Married	0.506	0.500	0.506	0.500	-0.000	0.506	0.500	-0.000
Divorced	0.181	0.385	0.181	0.385	0.000	0.181	0.385	0.000
Widowed	0.147	0.354	0.147	0.354	-0.000	0.147	0.354	-0.000
Primary Education	0.346	0.476	0.346	0.476	-0.000	0.346	0.476	-0.000
Secondary Education	0.421	0.494	0.421	0.494	0.000	0.421	0.494	0.000
Tertiary Education	0.233	0.423	0.233	0.423	0.000	0.233	0.423	0.000
Log Disp. Income	11.905	0.731	11.905	0.853	-0.000	11.905	0.808	0.000
HH Size	1.945	1.075	1.945	1.083	-0.000	1.945	1.096	0.000
Weighted Number of Observations	21737		21737			21737		

Note: SMD = Standardized Mean Difference compared to treatment group.

UPCI Before Exit = Share of contacts to most seen GP among all contacts before exit.

<sup>9</sup>We use the *ebalance* package for Stata (Hainmueller and Xu, 2013).

### 3.5 Outcome variables

Following the earlier literature, we examine the impact of GP exits on patients use of primary care, potential substitution to secondary care, and quality-related indicators. Given the low frequency of care use in Sweden, all outcomes are measured on an annual basis for the years 2009 to 2019.

The first set of outcomes covers primary care use. We consider face-to-face GP visits as the main outcome of interest but also study phone contacts, as Swedish GPs often handle follow-up consultations over the phone. We also study nurse visits, as nurses play an important role in Swedish primary care. For these outcomes, we consider both the intensive margins (number of yearly contacts) and extensive margins (at least one contact in a year).

The second set of outcomes relates to secondary care use: the number of specialist visits in outpatient clinics and an indicator for having had any planned hospitalization in a given year. Both these outcomes can be influenced by the new or old GP. Most previous studies in this literature find that specialist visits increase following exits,<sup>10</sup> suggesting new and old GPs are extra careful around the time of the exit. On the other hand, specialist visits can also decrease if access to primary care declines and patient see care at specialists instead, even though GPs are formally not gate-keepers in Sweden. From a health system perspective, such substitution would be especially concerning if it happens for patients with chronic conditions that are usually the responsibility of primary care; we therefore also examine the specialist visit outcome for this subgroup separately.<sup>11</sup>

Third, we use three quality-related outcomes. The first is an indicator for having filled at least one prescription of medication for chronic conditions typically handled by primary care.<sup>12</sup> This measure serves as a signal of disruptions of ongoing treatments and also captures changes in the proportion of the population with such a condition. The second is an indicator for any acute hospital stay. Acute hospitalizations may increase if GP exits reduce the quality of primary care. Finally, we examine an indicator for having a cancer diagnosis.<sup>13</sup> While the development of cancer is unrelated to the exit in the short term, previous research has shown that GP exits and the handover to a new GP may affect how fast the cancer is detected (Simonsen et al., 2021).

## 4 Estimation Strategy

We estimate the effect of GP exits in a difference-in-differences (DiD) framework. As GP exits occur at differed points in time, the treatment is "staggered". As such, the classical two-way fixed effects model with fixed effects for unit and calendar year may give misleading estimates (Roth et al., 2023). Further, individuals in the control groups have a (pseudo) treatment date and some

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<sup>10</sup>Bischof and Kaiser (2021); Sabety et al. (2021); Zhang (2022); Hjalmarsson et al. (2023); Sabety (2023); Simonsen et al. (2021); Zoher (2024); Monsees and Westphal (2025).

<sup>11</sup>Defined as having had at least one GP visit where a chronic condition was documented in  $t \in [-4, -2]$ . The set of chronic conditions we consider are: asthma, chronic obstructive pulmonary disease, depression, type 2 diabetes, heart failure, hypertension and ischemic heart disease. The respective ICD codes can be found in Table B.1 in the Appendix.

<sup>12</sup>The respective ATC codes can be found in Table B.1 in the Appendix.

<sup>13</sup>We include diagnoses for benign as well as malign tumors (ICD Chapters C, D1, D2, D3 and D4).

are included as controls in multiple exit years.<sup>14</sup> For these reasons, we use a stacked DiD approach, with one stack for each *exit year* (Wing et al., 2024).

Our model is specified as follows:

$$y_{its} = \alpha + \beta_1 \cdot Treated \cdot Year-1_t + \beta_2 \cdot Treated \cdot Post_t + \beta_3 \cdot Treated + \beta_4 \cdot Year-1_t + \beta_5 \cdot Post_t + \varepsilon_{its} \quad (1)$$

Here,  $y_{its}$  is the outcome variable of individual  $i$ , belonging to stack  $s$ , in relative year  $t$ .<sup>15</sup>  $\alpha$  is the constant term, which captures the pre-treatment mean outcome for the control group. The main coefficient of interest is the interaction term between the *Treated* indicator and the dummy variable *Post*, which equals one for observations from the exit year ( $t = 0$ ) onwards. In addition, we include a dummy variable *Year-1*, which equals one for observations in the last year before the exit ( $t = -1$ ) as well as its interaction with *Treated*. The reason why we estimate a separate coefficient for  $t = -1$  is that GPs or practices may adjust their behavior prior to the actual exit (Zocher, 2024).

Standard errors are clustered on the level of the main GP, as treatment is assigned at the GP level. The regression is weighted using the entropy weights derived in section 3.4. In robustness checks, we estimate unweighted models and try clustering standard errors at different levels, namely by PCC or by GP and individual.

Following results derived in Wing et al. (2024), our specification should yield unbiased estimates of the average treatment effect on the treated, because of two features of our sample. First, the sample is balanced over the observation window ( $t \in [-4, 4]$ ). Second, because we employ the entropy balancing within exit years, the resulting dataset has the same (weighted) number of observations in the treatment and control group within each stack. As a result there is no potential bias caused by differential weighting (based on sample shares) of treatment and control observations.

A crucial identifying assumption is nonetheless that treatment and control groups would have followed similar trajectories over time in the absence of the exit. To examine violations of this parallel trends assumption, we estimate event study models. These models also allow us to explore dynamic treatment effects, albeit at the cost of reduced statistical power. The stacked event study equation for outcome  $y$  of individual  $i$  in year  $t$  relative to the (placebo) event in stack  $s$  is specified as

$$y_{its} = \alpha_0 + \alpha_1 D_{is} + \sum_{k \in [-4, 4], k \neq [-3]} [\lambda_t 1[t = k] + \delta_t D_{is} \times 1[t = k]] + \varepsilon_{its} \quad (2)$$

where  $\alpha_0$  is a constant and  $\alpha_1 D_{is}$  indicates whether individual  $i$  belongs to the treatment group in stack  $s$ .  $\lambda_t$  captures estimates the relative time fixed effects while  $\delta_t$ , the parameter of interest, captures event study estimates only for the treated group.  $\varepsilon_{its}$  is the error term. Again, standard errors are clustered on the level of main GPs.

<sup>14</sup>The 60,395 observations in the non-exit practice control group represent 37,756 unique individuals. For the within practice control group, the corresponding figures are 28,787 and 19,811.

<sup>15</sup>Note that treated individuals are never included in the control group.

Pre-treatment violations of the parallel trends assumption can be examined by considering the  $\delta_t$  coefficients in the period leading up to treatment. Ideally, they should be zero until the onset of treatment. However, as the earlier literature suggests that GPs may change behavior already before the exit, we do not consider positive/negative values of  $\delta_{-1}$  as evidence of a violation of the parallel trends assumption.

## 5 Results

The first-order effects of GP exits in our study setting should be expected to manifest in patients' primary care use. We therefore consider this our main outcome and begin by examining how GP exits affect primary care use (section 5.1). Thereafter, we examine potential spillover effects on secondary care (section 5.2) and, lastly, if the exits have effects indicating changes in quality of care (section 5.3).

### 5.1 Primary care utilization

#### 5.1.1 Main results

We begin by presenting the event study estimates to explore pre-trends for the main outcomes of interest. In all figures, results for the non-exit practice sample are presented on the left and results for the within-practice sample on the right. The upper and lower part of each figure show the estimates for the intensive margins (number of contacts) and extensive margins (probability of contacts) respectively.

Figure 2 shows the estimates for face-to-face GP visits. As we included the number of visits in  $t \in [-4, -2]$  in the entropy balancing, there are mechanically no differential trends in that period irrespective of which control group we use (subfigures (a) and (b)). The estimates for the last year prior to the exit are also small and imprecise. Considering the extensive margin, i.e., the probability of at least one GP visit, the estimates are very close to zero in the pre-periods used for matching. The estimates are larger in absolute values in the last year prior to the exit, but statistically insignificant and very small in magnitude (-.01) in relation to the mean, as virtually everyone in our study population visits a GP yearly. Figures 3 and 4 show that there are also no indications of differential trends in the pre-period for the other two types of primary care-related outcomes, GP phone contacts and nurse visits.

Taken together, these analyses show no evidence of differential pre-trends in the weighted DiD approach. We therefore proceed to discuss the estimates of the effect of the GP exits. To gain precision, we focus on the results for the more high-powered DiD specification, which captures the effects on the whole post-period. Panel A of Table 3 shows the estimates using the non-exit practice control sample, which captures the combined effect of the breach in a GP-patient relationship and an increased workload on remaining GPs in the practice. Panel B shows the estimates using the within-practice control sample, which captures the part of the effect not driven by practice level

disruptions.

Irrespective of the control group, the DiD estimates on the number GP visits are statistically insignificant and very small relative to the control group mean in the pre-period, which is captured by the constant term. However, revisiting the event study graphs, it may be noted that the event study estimate for period  $t+1$  using the within-practice control group is statistically significant. When considering the extensive margin, there is a decline in the probability of a visit relative to the non-exit practice controls, but the magnitude is not economically meaningful – almost everyone in our sample visits a GP annually.

We find that patients affected by a GP exit have fewer GP phone contacts after the exit. The estimate is larger (more negative) when comparing to the non-exit practice controls than compared to the within-practice controls (  $-.27$  vs  $-.17$ ), suggesting the effect is a combination of both a workload effect and a direct effect of the breach in personal continuity. On the extensive margin, only the estimate comparing to the non-exit control sample – suggesting that exits reduce the probability of having a phone contact by 13% – is statistically significant. Taken together, the results for phone contacts suggest that the effect of the disrupted relationship is mainly a reduction in the volume of phone calls; it doesn't make it less likely to get at least one phone call. However, the increasing workload on remaining staff in the practice by itself reduces the chance of getting phone call, as well as the number of calls.

Further evidence that it is the workload rather than the relationship that matters are given by the estimates on nurse visits. There is a statistically significant increase in nurse visits when comparing to the non-exit practice control group, but not when comparing to the within-practice control group. This pattern is consistent with the story that exits lead to a higher workload for the remaining staff in the practice (Appendix A.1), which responds by task-shifting to nurses. Notably, the small and statistically insignificant estimate when comparing to the within-practice control group suggests that all patients in the practice where the exiting GP used to work are affected similarly by the exit. That is, the disruption of the GP-patient relationship itself has a negligible additional effect. This finding is also consistent with the idea that GPs in exit practice face a higher workload and thus shift tasks to other health care personnel, such as nurses.

### 5.1.2 Heterogeneity and robustness

To explore whether the estimates hide heterogeneity, we estimate the DiD models again for subgroups defined by age (at most versus at least 67 years), sex, existence of a chronic condition diagnosis (before the exit), and educational attainment (at most primary, secondary, tertiary level). We only present the estimates for  $\beta_2$  from the DiD Equation 1, as they are the main estimates of interest.

Figure 5 shows that the estimates on the effect on the number of GP visits are very stable across all subgroups when using the non-exit practice control group. The results are also quite homogeneous when using the within-practice control group. The exception is that for patients with no pre-existing chronic condition, the effect is larger and statistically significant. We observe around .15 additional visits after the exit. There is also a tendency of an educational gradient, with large (more positive)

estimates for patients with more education

The subgroup estimates for phone contacts are a bit more heterogeneous than for visits, but all are negative, and like the main estimate, many are statistically significant, irrespective of the control group used (Figure 6).

For nurse visits (Figure 7), the positive main effect relative to the non-exit practice control group is quite similar across the subgroups. In the specification using the within-practice controls, the subgroup analysis uncovers more heterogeneity in the point estimates. Especially notable is the relatively large positive estimate for patients with the lowest educational attainment (at most primary education). Placing this result side by side with the heterogeneity in the effect on GP visits, the pattern indicates that nurses take on a greater responsibility for patients with low education following the loss of their GP, whereas highly educated patients are handled by GPs to a greater extent. Given the noisy estimates, we caution against over-emphasizing the heterogeneity, however.

We also examine robustness of our main findings in various ways (Figures 5c-5d, 6c-6d and 7c-7d). First, we estimate the models without using the entropy weights. They barely influence the estimates from the specification using the non-exit practice control group, for either GP visits, phone contacts or nurse visits. When using the within-practice control group, the unweighted specification yields a negative and statistically significant coefficient on the number of GP visits. We strongly favor the weighted specification, for reasons explained in section 3.2, the subset of patients with meaningful relationships to exiting GPs had unusually high levels of primary care use in the pre-period, which mechanically induces mean reversion thereafter. As illustrated in Table 1 and Figure A.8, the unweighted within-practice control group was on a much lower level initially, and therefore mechanically experience a smaller fall in  $t = -1$  onwards. The negative unweighted coefficient in Fig. 5b thus mainly captures a mechanical, and uninformative, effect.<sup>16</sup>

Second, we check if the main estimates are driven by specific exit years by estimating separate models for each exit year. These specifications are naturally noisy, as the number of observations is much smaller. For the models using the non-exit practice control group, the estimates on GP visits have mixed signs but all are statistically insignificant except the positive estimate for 2009. The corresponding estimate is also positive and statistically significant when using the within-practice control group. Given that all other years have smaller and statistically insignificant coefficients, we view 2009 as an exception rather than the rule. 2009 was also a special year in the study region due to a large number of new PCCs opening up following a deregulation reform. Thus, the characteristics of exiting GPs was likely different than in other years. The estimates for phone contacts and nurse visits vary over the years. Notably, the negative impact on phone contacts in the within-practice sample appears to be driven by the earlier exit years.

Third, we check if the inference is robust to using other clustering levels. One could argue that the best level of clustering would be to cluster on both main GP and exit year. The reason why we do not prefer this approach is that it fails to account for the fact that the same control patient may appear in multiple stacks. Another plausible approach would be to cluster at the practice (PCC) level, which may make sense as the whole practice is affected when an important and relatively productive GP quits. Fig. 5c and 5d show that the level of clustering is not crucial for

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<sup>16</sup>For the non-exit practice control group, the unweighted level difference is much smaller, hence the weights do not make such a big difference.

our conclusions regarding the null impact on GP visits. However, the estimates on phone contacts and nurse visits are somewhat sensitive to the level of clustering, especially in the specifications using the non-exit practice control group. While we still prefer the main specification, the results indicating increased nurse visits and reduced phone contacts should be viewed as tentative.

As a fourth robustness check, we change the study population definition by retaining patients who dropped out of the sample during the post period in the sample. The estimates are barely, if at all, affected, indicating that differential attrition is not an issue.

Fifth and finally, we examine if the generally small effects on primary care use reflect a more fundamental issue with our estimation strategy: the fact that the control group may experience a drop in continuity of care in the post period as well. Even though their main GP remained in the practice, the weak formal links between patients and GPs in the Swedish setting means that there is no guarantee that they continued to have that person as their main GP throughout the follow-up period. In Table A.8, we estimate simple two-period DiD models of three established measures on continuity of care. All models give similar results. To focus on one measure, the usual provider of care index (UPCI) – the share of visits with the most commonly seen GP: we find that the control groups indeed experienced a drop in the mean UPCI between the pre and post period. For instance, for the non-exit practice control sample, the UPCI decreased by 15 percentage points from a baseline of 73 percent. However, the decline was 10 percentage point larger for the treatment group. That is, in support of our estimation strategy, the treatment group experienced a considerably larger decline in continuity of care. In addition, and in difference to the control group, they no longer had any access to their previous main GP.

## 5.2 Secondary care utilization

The analysis so far does not indicate that the disruption itself has a major impact on the primary care use of patients whose GP exited. However, it is still possible that the content or patients' perception of the service changed, leading them to consult specialists to a smaller or larger degree than before. Further, new and old GPs may have different practice styles, including propensities to refer patients to secondary care. In this section, we examine if there is evidence of such substitution between primary and secondary care. We consider outpatient specialist visits in general and only among individuals with chronic conditions at the exit and planned hospital stays. Table 4 shows the DiD estimates, the event study estimates are included in Appendix A.4. We do not observe any effects on specialist visits for the whole sample. There is, however, an increase in specialist visits among chronically ill in the non-exit practice comparison amounting to a 3.5% increase relative to the mean. As there is no similar impact in the comparison with the within-practice control group, this potential substitution affect appears to reflect lower access due to a higher workload in primary care, rather than being a result of the disrupted GP-patient relationship. We find no effect on the probability of a non-acute hospitalization.

### 5.3 Health-related outcomes

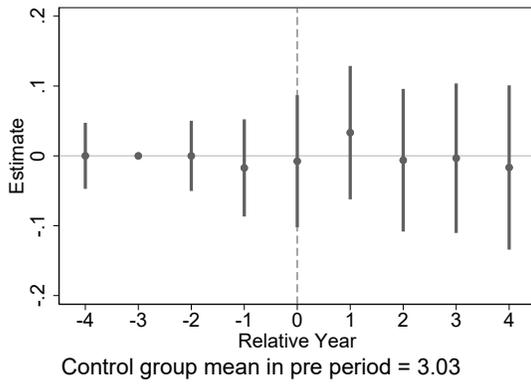
Finally, we examine whether the exits have any effects on quality-related outcomes (Table 5). We observe almost no statistically significant estimate across outcomes and samples. The one exception is acute hospital stays, for which there is a positive and statistically significant estimate on the last year prior to the exit in the specification using the non-exit practice control group. As the coefficient is only statistically significant at the 10% level, we interpret this result cautiously and view it as a potential chance finding.

Across the board, the coefficients are small in size. For the prescription outcome, even the lower boundaries of the 95% confidence intervals are small enough to rule out economically meaningful impacts relative to the mean prescription rate of 72%. For the other two outcomes, which are rarer, the 95% confidence intervals include relative effects that may sound notable. For instance, in the within-practice control group, the upper bound of the confidence interval suggest that we cannot rule out a 6% increase relative to the mean likelihood of an acute hospital stay. However, an increase from .106 to .112 is clearly not clinically significant.

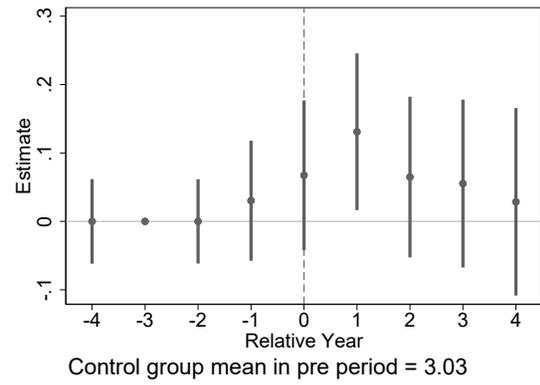
In relation to the previous literature, it is notable that we do not find evidence of increased cancer detection or changes in the uptake of medications for common chronic conditions. These findings align with the idea that the Swedish way of organizing primary care makes the exit of specific GPs less crucial than in settings where GPs carry the full responsibility for their patients.

Figure 2: Event Study Estimates: GP Visits

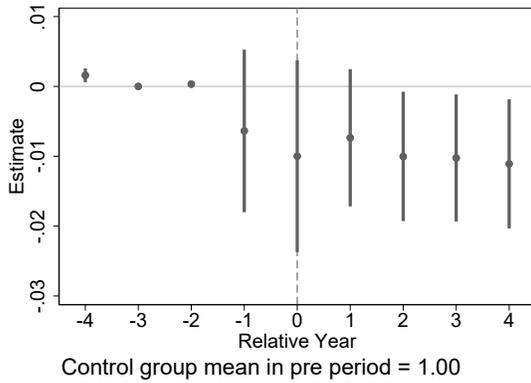
(a) Non-Exit Practice Control:  
Number of GP Visits



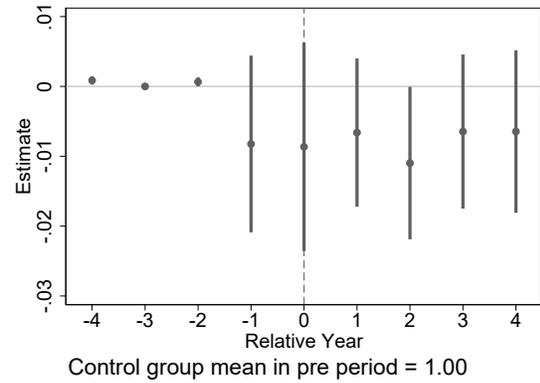
(b) Within Practice Control:  
Number of GP Visits



(c) Non-Exit Practice Control:  
Any GP Visit



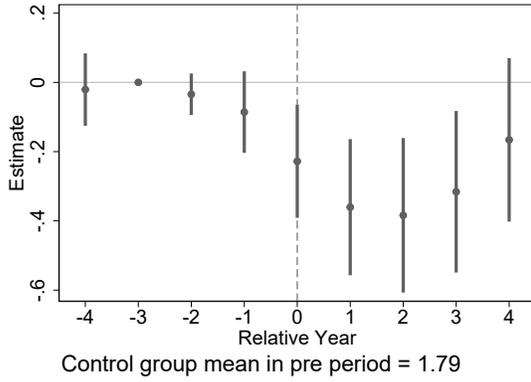
(d) Within Practice Control:  
Any GP Visit



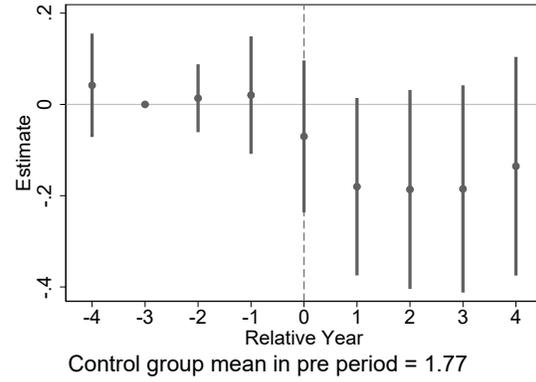
Note: Entropy weighted event study estimates.

Figure 3: Event Study Estimates: GP Phone Contacts

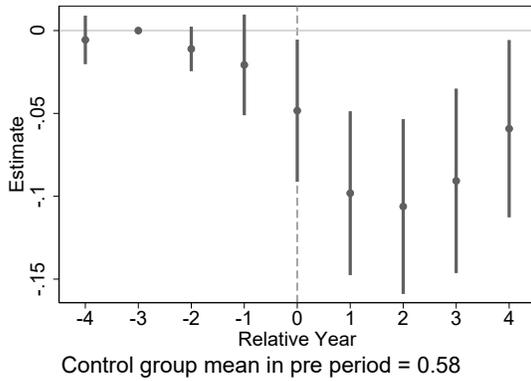
(a) Non-Exit Practice Control:  
Number of GP Phone Contacts



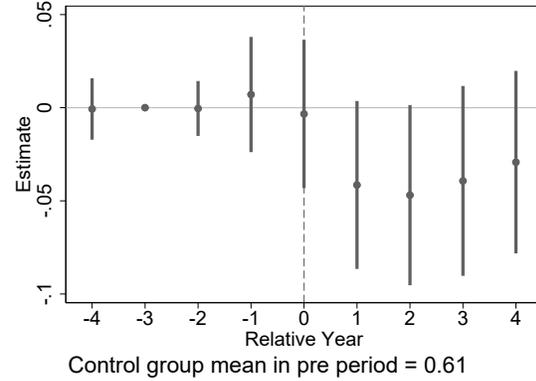
(b) Within Practice Control:  
Number of GP Phone Contacts



(c) Non-Exit Practice Control:  
Any GP Phone Contact



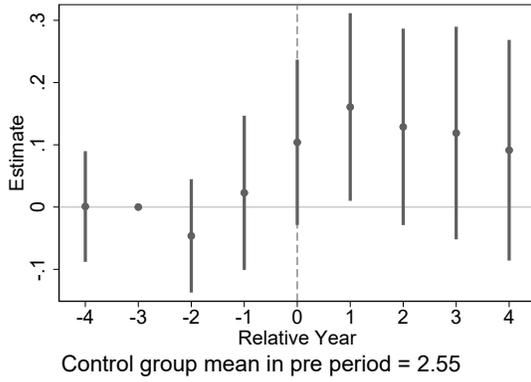
(d) Within Practice Control:  
Any GP Phone Contact



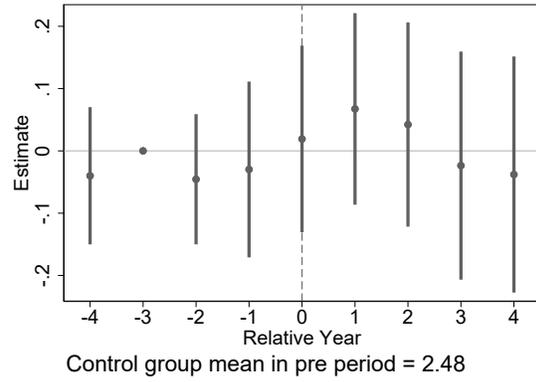
Note: Entropy weighted event study estimates.

Figure 4: Event Study Estimates: Nurse Visits

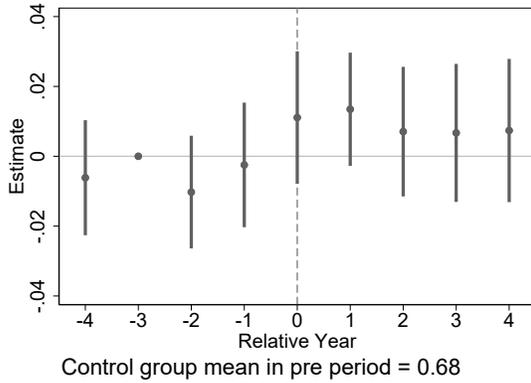
(a) Non-Exit Practice Control:  
Number of Nurse Visits



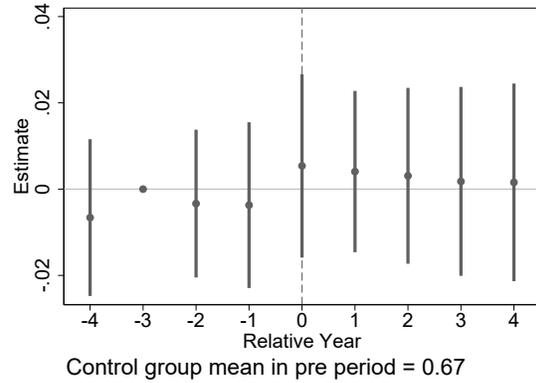
(b) Within Practice Control:  
Number of Nurse Visits



(c) Non-Exit Practice Control:  
Any Nurse Visit



(d) Within Practice Control:  
Any Nurse Visit



Note: Entropy weighted event study estimates.

Table 3: DiD Estimates Primary Care Utilization

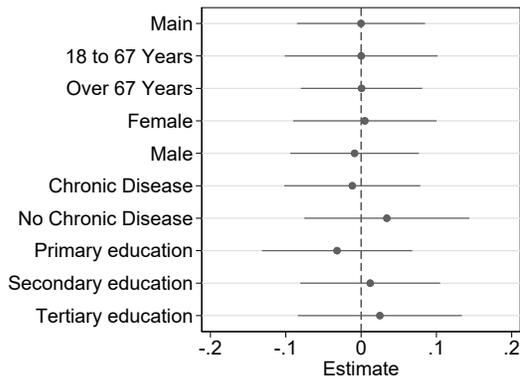
	GP Visits	Any GP Visits	GP Phone Contacts	Any GP Phone Contact	Nurse Visits	Any Nurse Visit
<i>A. Non-Exit Practice Control</i>						
Year -1	-0.333*** (0.0170)	-0.105*** (0.00283)	-0.187*** (0.0290)	-0.0354*** (0.00705)	0.0657** (0.0267)	0.00673** (0.00342)
Post Period	-0.360*** (0.0242)	-0.122*** (0.00259)	-0.147** (0.0639)	-0.0267* (0.0146)	0.168*** (0.0452)	0.0125*** (0.00421)
Treated	0.0000967 (0.0522)	0.000730** (0.000283)	0.0231 (0.111)	0.0229 (0.0246)	-0.0253 (0.0909)	0.00126 (0.0117)
DiD Estimate Year -1	-0.0172 (0.0313)	-0.00701 (0.00593)	-0.0675 (0.0562)	-0.0152 (0.0145)	0.0380 (0.0536)	0.00299 (0.00686)
DiD Estimate Post Period	-0.000135 (0.0433)	-0.0104** (0.00429)	-0.273*** (0.102)	-0.0749*** (0.0245)	0.136** (0.0664)	0.0146** (0.00711)
Constant	3.029*** (0.0357)	0.999*** (0.000260)	1.795*** (0.0811)	0.585*** (0.0164)	2.552*** (0.0717)	0.676*** (0.00828)
Num Obs	739188	739188	739188	739188	739188	739188
Exit Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
PCC FEs	Yes	Yes	Yes	Yes	Yes	Yes
<i>B. Within Practice Control</i>						
Year -1	-0.381*** (0.0293)	-0.103*** (0.00371)	-0.256*** (0.0343)	-0.0581*** (0.00732)	0.105** (0.0450)	0.0101** (0.00451)
Post Period	-0.430*** (0.0357)	-0.124*** (0.00371)	-0.249*** (0.0593)	-0.0699*** (0.0104)	0.262*** (0.0530)	0.0206*** (0.00558)
Treated	0.000326 (0.0600)	0.000283 (0.000247)	0.0448 (0.113)	0.00144 (0.0238)	0.0452 (0.0829)	0.00667 (0.0115)
DiD Estimate Year -1	0.0302 (0.0397)	-0.00874 (0.00643)	0.00187 (0.0593)	0.00745 (0.0147)	-0.00134 (0.0642)	-0.000394 (0.00740)
DiD Estimate Post Period	0.0694 (0.0509)	-0.00834 (0.00506)	-0.170* (0.0994)	-0.0317 (0.0222)	0.0419 (0.0716)	0.00649 (0.00793)
Constant	3.029*** (0.0457)	1.000*** (0.000221)	1.773*** (0.0822)	0.606*** (0.0151)	2.482*** (0.0607)	0.671*** (0.00785)
Num Obs	450135	450135	450135	450135	450135	450135
Exit Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
PCC FEs	Yes	Yes	Yes	Yes	Yes	Yes

*Note:* Constant represents the mean of the control group in the pre-period. The standard errors (in parentheses) are clustered on the main GP level.

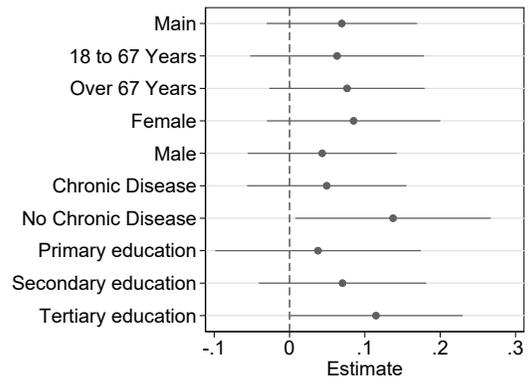
\* P<0.1, \*\* P<0.05, \*\*\* P<0.01.

Figure 5: Heterogeneities: Number of GP visits

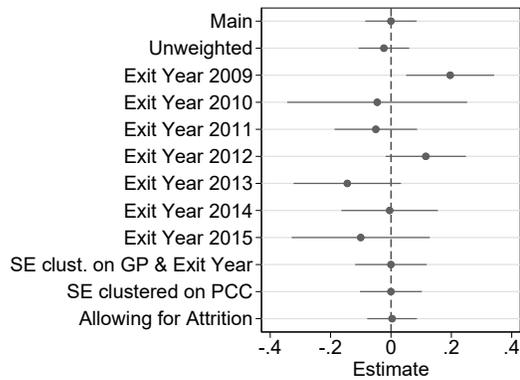
(a) Non-Exit Practice: Subgroups



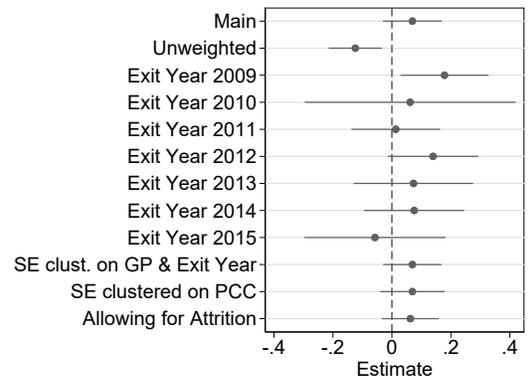
(b) Within Practice: Subgroups



(c) Non-Exit Practice: Sample Selection



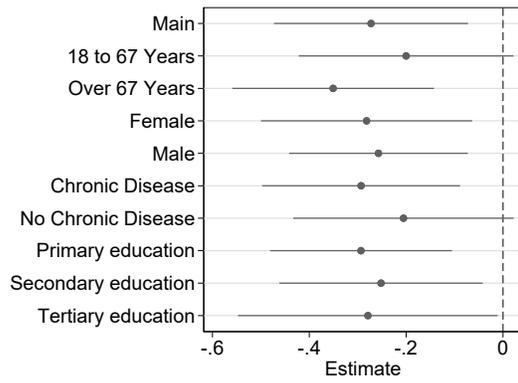
(d) Within Practice: Sample Selection



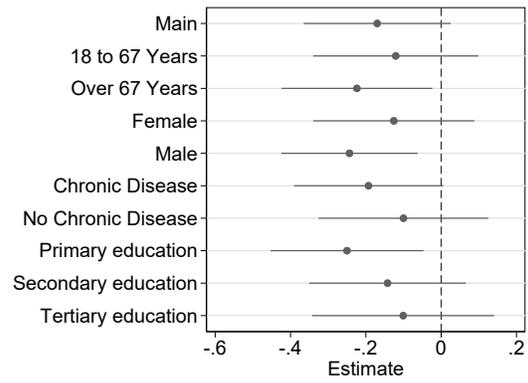
Note: Presented are the estimates of *DiD Estimate Post Period* following Table 3.

Figure 6: Heterogeneities: Number of GP phone contacts

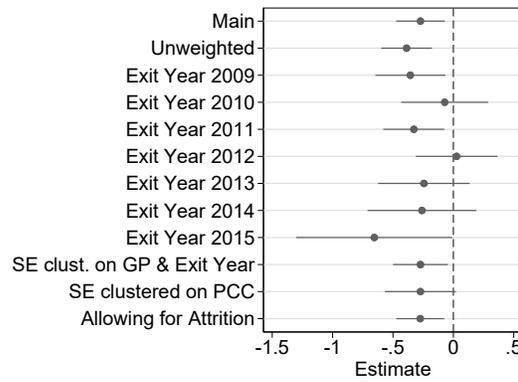
(a) Non-Exit Practice: Subgroups



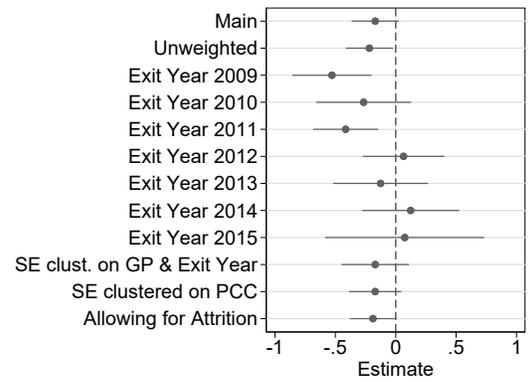
(b) Within Practice: Subgroups



(c) Non-Exit Practice: Sample Selection



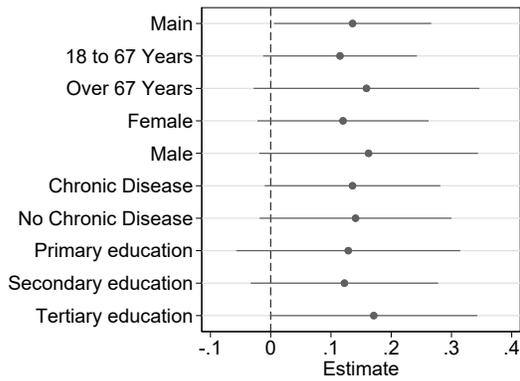
(d) Within Practice: Sample Selection



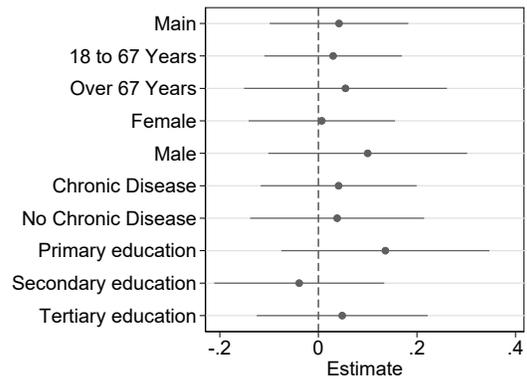
Note: Presented are the estimates of *DiD Estimate Post Period* following Table 3.

Figure 7: Heterogeneities: Number of nurse visits

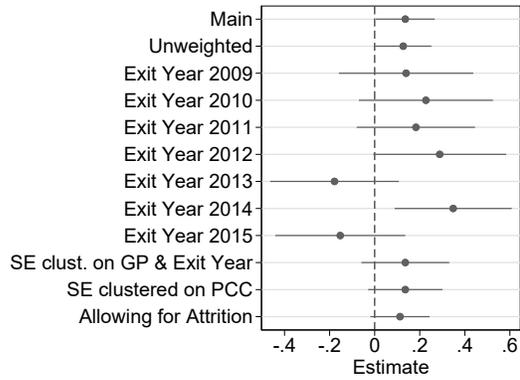
(a) Non-Exit Practice: Subgroups



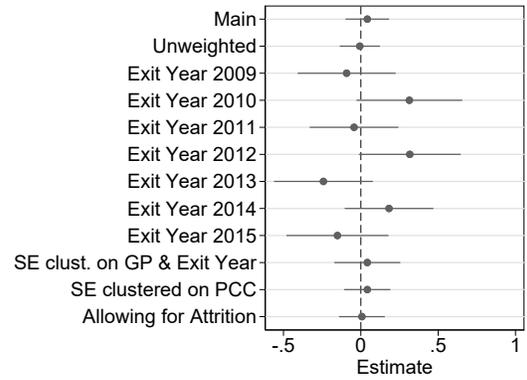
(b) Within Practice: Subgroups



(c) Non-Exit Practice: Sample Selection



(d) Within Practice: Sample Selection



Note: Presented are the estimates of *DiD Estimate Post Period* following Table 3.

Table 4: DiD Estimates Secondary Care Utilization

	Specialist Visits	Specialist Visits Patients with Chronic Conditions	Any Non-Acute Hospital Stays
<i>A. Non-Exit Practice Control</i>			
Year -1	0.0118 (0.0131)	0.0170 (0.0163)	-0.000717 (0.00110)
Post Period	0.129*** (0.0194)	0.160*** (0.0232)	-0.00454*** (0.00115)
Treated	0.0240 (0.0438)	0.0253 (0.0493)	-0.00104 (0.00162)
DiD Estimate Year -1	0.0177 (0.0246)	0.0384 (0.0294)	0.000993 (0.00219)
DiD Estimate Post Period	0.0377 (0.0316)	0.0729** (0.0365)	0.00145 (0.00165)
Constant	2.055*** (0.0298)	2.112*** (0.0339)	0.0558*** (0.00115)
Num Obs	739188	492489	739188
Exit Year FEs	Yes	Yes	Yes
PCC FEs	Yes	Yes	Yes
<i>B. Within Practice Control</i>			
Year -1	0.0143 (0.0221)	0.0520* (0.0286)	0.00347** (0.00160)
Post Period	0.183*** (0.0246)	0.238*** (0.0313)	-0.00260** (0.00123)
Treated	0.101** (0.0486)	0.114** (0.0534)	0.00190 (0.00165)
DiD Estimate Year -1	0.0152 (0.0307)	0.00330 (0.0378)	-0.00320 (0.00248)
DiD Estimate Post Period	-0.0160 (0.0355)	-0.00566 (0.0432)	-0.000494 (0.00169)
Constant	1.978*** (0.0361)	2.023*** (0.0397)	0.0528*** (0.00117)
Num Obs	450135	318258	450135
Exit Year FEs	Yes	Yes	Yes
PCC FEs	Yes	Yes	Yes

*Note:* Constant represents the mean of the control group in the pre-period. The standard errors (in parentheses) are clustered on the main GP level.

\* P<0.1, \*\* P<0.05, \*\*\* P<0.01.

Table 5: DiD Estimates Quality Indicators

	Any Prescription for Chronic Conditions	Any Acute Hospital Stays	Any Cancer Diagnosis
<i>A. Non-Exit Practice Control</i>			
Year -1	0.0109*** (0.00162)	0.00190 (0.00137)	0.0127*** (0.00108)
Post Period	0.0384*** (0.00214)	0.0203*** (0.00148)	0.0365*** (0.00165)
Treated	0.00334 (0.00972)	-0.00205 (0.00258)	0.000707 (0.00289)
DiD Estimate Year -1	0.000765 (0.00286)	0.00511* (0.00280)	-0.00150 (0.00204)
DiD Estimate Post Period	-0.000809 (0.00342)	0.00323 (0.00236)	-0.00163 (0.00242)
Constant	0.720*** (0.00613)	0.112*** (0.00184)	0.0662*** (0.00190)
Num Obs	739188	739188	739188
Exit Year FEs	Yes	Yes	Yes
PCC FEs	Yes	Yes	Yes
<i>B. Within Practice Control</i>			
Year -1	0.0131*** (0.00223)	0.00633*** (0.00230)	0.0104*** (0.00157)
Post Period	0.0402*** (0.00261)	0.0233*** (0.00224)	0.0359*** (0.00199)
Treated	0.00579 (0.0107)	0.00426 (0.00288)	-0.00220 (0.00394)
DiD Estimate Year -1	-0.00143 (0.00327)	0.000678 (0.00334)	0.000747 (0.00235)
DiD Estimate Post Period	-0.00262 (0.00374)	0.000243 (0.00288)	-0.000977 (0.00266)
Constant	0.718*** (0.00746)	0.106*** (0.00223)	0.0691*** (0.00327)
Num Obs	450135	450135	450135
Exit Year FEs	Yes	Yes	Yes
PCC FEs	Yes	Yes	Yes

*Note:* *Constant* represents the mean of the control group in the pre-period. The standard errors (in parentheses) are clustered on the main GP level.

\* P<0.1, \*\* P<0.05, \*\*\* P<0.01.

## 6 Discussion

We study the impact of GP exits in the Swedish context. In this setting, primary care is organized in group practices where patients commonly interact with multiple GPs. Examining the effect of GP exits in such a context is itself a contribution to the literature. Additionally, we provide novel insights by using two control groups that allow us to distinguish the effect of the break in the patient-provider relationship from the overall practice-level disruptions caused by the exit of a GP.

The results suggest that GP exits affect patients little, if at all, in the Swedish primary care setting. While we do find some statistically significant changes in primary care use when comparing patients affected by the exit of their main GP to patients in other practices, the effects are always smaller and usually statistically insignificant when comparing the treatment group to patients in the same practice, whose main GP stayed. The contrast between the two sets of results suggests that the most important effect of a GP exit relates to the capacity constraints facing the practice after the exit of an experienced GP. The disruption of the GP-patient relationship *per se* does not seem to affect primary care use substantially, and does not affect the use of secondary care at all. In contrast to previous studies, we find no evidence of increased hospitalizations due to the exit, and no increase in cancer diagnoses or prescriptions for chronic conditions.

The only evidence of pure disruption effects – effects relative to the within-practice control group – concerns phone consultations with a 10% decrease relative to the mean. It is balanced by a short-term increase in the number of GP visits (Figure. 2d). This could reflect that exiting GPs have a different practice style than the remaining and newly hired GP, or that the new GPs want to meet the new patients in-person in situations where the main GP would have felt safe to handle the issue over the phone. Given the lack of impact on secondary care use and quality-related outcomes, these changes do not seem to be of clinical importance.

The effects relative to the non-exit practice control group suggest that the increased workload on the remaining GP in the practices affected by an exit has more far-reaching impacts on care use patterns. The exits increase task-shifting to nurses and, for patients with a pre-existing chronic condition, secondary care specialists. The lack of impact on acute hospital stays suggests that the task-shifting has no evident impact on quality, though.

A notable limitation of our approach is that the study sample differs from the general Swedish population, as the included individuals have a relatively high number of GP visits – and high continuity of care – prior to the exit. However, this is a direct consequence of our sample selection criteria, which were designed to exclude individuals with no meaningful relationship to exiting GPs. As a result, our estimates represent an upper bound on the possible effects of disruptions of the patient-provider relationship. Viewed from another angle, had the excluded patients had a similarly firm relationship with a main GP, we have no reason to believe that expect that the consequences of their GP's exit would have differed.

A related caveat is that analysis is based on a special group of GPs. Swedish GPs commonly move between different PCCs, and rarely work in the same practice for four years. This indicates that our exiting GPs fill a special role in their workplace, which is also supported by the relatively high workload carried by these GPs as shown in Figure A.1a.

A third limitation is that we are not able to directly observe continuity with other practice staff, especially nurses, which may play an important role in continuity of care. However, for the purpose of examining the impact of a breach in GP-patient relationships, such data is not necessary.

## 7 Concluding remarks

Our results are consistent with the expectation that GP exits are insignificant events in the Swedish primary care setting, where patients usually meet different GPs even if they have a main GP, and where their practice is responsible for organizing a replacement GP after an exit. Importantly, the insignificance of GP exits cannot be explained by low continuity of care at baseline, as our study population only includes individuals with meaningfully long relationships to the exiting GPs.

Our results contrast with the results from several other countries, where disruptions have been shown to have both negative (e.g. increased hospitalizations) and positive effects (e.g. faster detection of diagnoses). In most of those settings, the effects are attributed to the loss of the relationship rather than to reduced access to primary care, as patients tend to find a new GP within a short period. Our results highlight that a wider exposure to different GPs in normal times can lessen the importance of the personal relationship and mitigate the consequences of GP exits. In our setting, it is common for patients to be examined by different GPs and thus the new GP seen just after an exit is no different from the new GP seen at other occasions.

In addition to making patients less vulnerable to the exit of their main GP, the Swedish way of organizing primary care, with GPs working as salaried employees in multi-professional group practices, may also reduce GP loneliness and financial stress – factors that have been identified as contributing to the current GP shortage facing countries. That being said, GP turnover is high in Sweden, possibly reflecting the other side of the coin: with salaried employment, GPs face low transaction costs from switching workplace. Further, many Swedes experience that the continuity of care is very low. Even though our results indicate that it is possible to mitigate the importance on personal continuity at the time of a GP exit, continuity is still a feature that is valued by patients and considered a hallmark of high-quality primary care by the international GP community. How to make patients resilient to the negative impact of GP exits disruptions while still ensuring an adequate level of continuity is a fruitful topic for future research on the design of primary care.

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# Appendix

## A Additional Results

### A.1 Capacity Constraints Following Exits

To give some insights into what the loss of the exiting GPs means to affected practices we provide several descriptive statistics (Figure A.1). Figure A.1a shows that the exiting GPs provide around 900 visits per year before their exit. This is twice as much as the average of all other GPs in the PCCs ("staying GPs"). An explanation might be that the exiting GPs have been working in the PCCs for four years or more and are therefore more involved in the practice (compared to the large number of GPs who often switch PCCs). In line with this, staying long term GPs (working in the PCC for four years or more but not exiting in the exit year) provide more visits than staying GPs on average. New GPs (GPs starting their PCC spell in the exit year or later) provide much fewer visits than the exiting GPs they are supposedly replacing. Compared to the remaining GPs, the exiting GPs thus provide significantly more services. This further translates to exiting GPs being responsible for a large fraction of overall visits provided within PCC (Figure A.1b). It is therefore reasonable to assume that the exits of the GPs on our setting is a significant event on the practice level. This conclusion is also confirmed when comparing the practices with exiting GPs (Exit Practices) with those where no GP leaves (Non-Exit Practice).<sup>17</sup> In Figure A.1c it can be seen that exit practices provide more visits before the exit than non-exit practices. However, this difference disappears after the exit. Still, the exiting practices employ more GPs (Figure A.1d), contributing to the idea that the new GPs do not provide the same volume of services as the exiting GPs.<sup>18</sup>

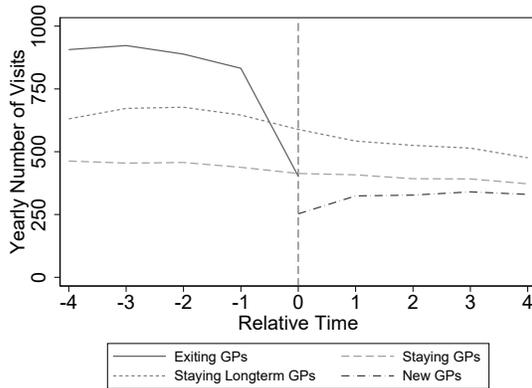
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<sup>17</sup>The non-exit practices are the practices of the non-exit practice control group with the same pseudo treatment date.

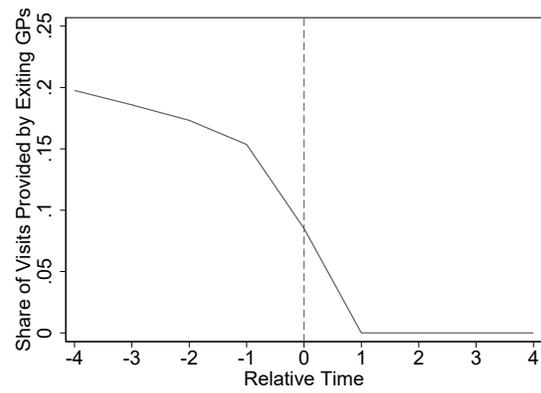
<sup>18</sup>Note that the increase in relative year 0 is due to the fact that all GPs working at any time in the PCC are included. E.g. if a GP exits in June and is replaced by a new GP in July, this is counted as two GPs.

Figure A.1

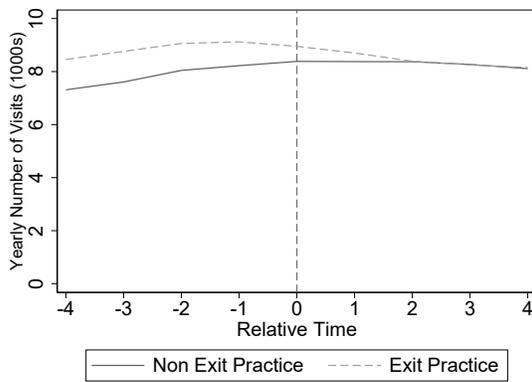
(a) Number of visits by GP type (PCC with exits)



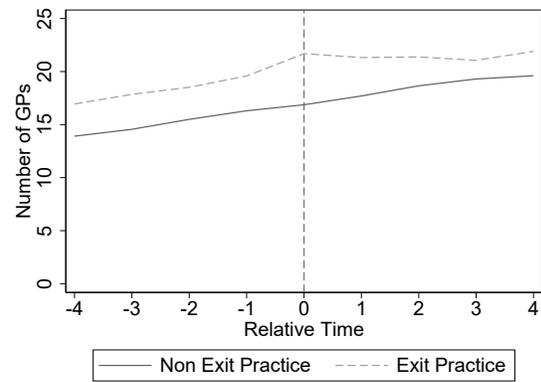
(b) Contribution of exiting GPs to PCC workload



(c) Number of visits by practice type



(d) Number of GPs by practice type



Note: Yearly averages.

## A.2 Additional Descriptives

Table A.1: Descriptives: Outcome Variables Pre Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
GP Visits	3.029	2.333	2.936	2.274	0.040	2.600	1.925	0.200
Any GP Visits	1.000	0.015	0.999	0.030	0.030	1.000	0.017	0.004
GP Phone Contacts	1.818	2.636	1.669	2.593	0.057	1.635	2.377	0.073
Any GP Phone Contacts	0.608	0.488	0.553	0.497	0.110	0.585	0.493	0.045
Nurse Visits	2.527	5.164	2.493	5.315	0.006	2.311	4.811	0.043
Any Nurse Visits	0.677	0.467	0.672	0.469	0.011	0.662	0.473	0.034
Specialist Visits	2.079	3.442	2.011	3.307	0.020	1.784	2.924	0.092
Chronic Specialist Visits	2.137	3.597	2.070	3.447	0.019	1.820	2.997	0.096
Chronic Condition before Exit	0.692	0.462	0.657	0.475	0.075	0.719	0.450	-0.059
Any Non-Acute Hospital Stay	0.055	0.227	0.055	0.227	0.000	0.049	0.217	0.024
Any Chronic Prescription	0.723	0.447	0.718	0.450	0.012	0.728	0.445	-0.010
Any Acute Hospital Stay	0.110	0.313	0.110	0.313	0.000	0.095	0.293	0.050
Any Cancer Diagnosis	0.067	0.250	0.067	0.249	0.001	0.070	0.254	-0.011
Number of Observations	65211		181185			84834		

*Note:* SMD = Standardized Mean Difference compared to treatment group. *Chronic Specialist Visits* = Specialist visits of individuals with chronic conditions before the exit. Included are the Years -4 to -2 before the Exit.

Table A.2: Descriptives: Outcome Variables Post Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
GP Visits	3.029	2.333	3.029	2.463	0.000	3.029	2.453	0.000
Any GP Visits	1.000	0.015	0.999	0.031	0.030	1.000	0.022	0.015
GP Phone Contacts	1.818	2.636	1.795	2.711	0.009	1.773	2.547	0.017
Any GP Phone Contacts	0.608	0.488	0.585	0.493	0.047	0.606	0.489	0.003
Nurse Visits	2.527	5.164	2.552	5.594	-0.005	2.482	5.208	0.009
Any Nurse Visits	0.677	0.467	0.676	0.468	0.003	0.671	0.470	0.014
Specialist Visits	2.079	3.442	2.055	3.393	0.007	1.978	3.164	0.031
Chronic Specialist Visits	2.137	3.597	2.112	3.531	0.007	2.023	3.251	0.033
Chronic Condition before Exit	0.692	0.462	0.677	0.468	0.032	0.711	0.454	-0.041
Any Non-Acute Hospital Stay	0.055	0.227	0.056	0.230	-0.005	0.053	0.224	0.008
Any Chronic Prescription	0.723	0.447	0.720	0.449	0.007	0.718	0.450	0.013
Any Acute Hospital Stay	0.110	0.313	0.112	0.316	-0.007	0.106	0.308	0.014
Any Cancer Diagnosis	0.067	0.250	0.066	0.249	0.003	0.069	0.254	-0.009
Weighted Number of Observations	65211		65211			65211		

Note: SMD = Standardized Mean Difference compared to treatment group. Included are the Years -4 to -2 before the Exit.

Table A.3: Descriptives: Diagnoses Pre Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
Any GP Contacts Asthma	0.047	0.211	0.046	0.210	0.003	0.048	0.214	-0.005
Any GP Contacts Depression	0.121	0.326	0.112	0.315	0.028	0.112	0.316	0.027
Any GP Contacts T2 Diabetes	0.132	0.339	0.124	0.329	0.025	0.144	0.351	-0.034
Any GP Contacts Hypertension	0.336	0.472	0.331	0.471	0.011	0.380	0.486	-0.093
Any GP Contacts Heartfailure	0.009	0.094	0.008	0.086	0.015	0.007	0.086	0.017
Any GP Contacts COPD	0.030	0.170	0.027	0.161	0.020	0.028	0.165	0.011
Any GP Contacts IHD	0.071	0.257	0.073	0.261	-0.008	0.078	0.268	-0.024
Any Contacts Cancer	0.067	0.250	0.067	0.249	0.001	0.070	0.254	-0.011
Any Prescr of any Chron Drug	0.723	0.447	0.718	0.450	0.012	0.728	0.445	-0.010
Number of Observations	65211		181185			84834		

Note: SMD = Standardized Mean Difference compared to treatment group. Included are the Years -4 to -2 before the Exit.

Table A.4: Descriptives: Diagnoses Post Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
Any GP Contacts Asthma	0.047	0.211	0.048	0.213	-0.005	0.051	0.220	-0.020
Any GP Contacts Depression	0.121	0.326	0.115	0.319	0.017	0.124	0.329	-0.009
Any GP Contacts T2 Diabetes	0.132	0.339	0.128	0.334	0.012	0.136	0.343	-0.012
Any GP Contacts Hypertension	0.336	0.472	0.338	0.473	-0.005	0.355	0.479	-0.041
Any GP Contacts Heartfailure	0.009	0.094	0.008	0.088	0.011	0.007	0.085	0.019
Any GP Contacts COPD	0.030	0.170	0.028	0.165	0.012	0.030	0.169	0.002
Any GP Contacts IHD	0.071	0.257	0.075	0.264	-0.015	0.074	0.261	-0.008
Any Contacts Cancer	0.067	0.250	0.066	0.249	0.003	0.069	0.254	-0.009
Any Prescr of any Chron Drug	0.723	0.447	0.720	0.449	0.007	0.718	0.450	0.013
Weighted Number of Observations	65211		65211			65211		

*Note:* SMD = Standardized Mean Difference compared to treatment group. Included are the Years -4 to -2 before the Exit.

Table A.5: Descriptives: Continuity Measures Pre Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
UPC Index in Pre Period	0.704	0.204	0.732	0.194	-0.141	0.771	0.178	-0.350
Share of visits with Long Term GP	0.848	0.178	0.876	0.157	-0.163	0.900	0.133	-0.326
Continuity of Care Index	0.553	0.249	0.584	0.247	-0.126	0.633	0.238	-0.329
Sequential Continuity of Care Index	0.623	0.188	0.628	0.189	-0.025	0.669	0.178	-0.252
Number of Different GPs	4.608	2.866	4.288	2.754	0.114	3.799	2.417	0.305
Spell duration Main GP	2368.212	659.688	4685.964	855.039	-3.035	4960.304	685.206	-3.854
Main GP was only GP	0.160	0.367	0.181	0.385	-0.055	0.235	0.424	-0.189
Number of Observations	21737		60395			28278		

*Note:* SMD = Standardized Mean Difference compared to treatment group. Based on the Years -4 to -1 before the Exit.

Table A.6: Descriptives: Continuity Measures Post Balancing

	TREATED		NON-EXIT PRACTICE CONTROL			WITHIN PRACTICE CONTROL		
	Mean	SD	Mean	SD	SMD	Mean	SD	SMD
UPC Index in Pre Period	0.704	0.204	0.730	0.195	-0.133	0.727	0.193	-0.117
Share of visits with Long Term GP	0.848	0.178	0.877	0.157	-0.168	0.879	0.150	-0.183
Continuity of Care Index	0.553	0.249	0.582	0.247	-0.116	0.578	0.245	-0.100
Sequential Continuity of Care Index	0.623	0.188	0.628	0.188	-0.028	0.637	0.184	-0.073
Number of Different GPs	4.608	2.866	4.399	2.837	0.073	4.436	2.828	0.060
Spell duration Main GP	2368.212	659.688	4775.527	797.944	-3.288	4951.536	690.667	-3.825
Main GP was only GP	0.160	0.367	0.168	0.374	-0.021	0.162	0.368	-0.005
Weighted Number of Observations	21737		21737			21737.000		

*Note:* SMD = Standardized Mean Difference compared to treatment group. Based on the Years -4 to -1 before the Exit.

Table A.7: Descriptives: Continuity Measures Pre Balancing

	TREATED		GENERAL POPULATION		SMD
	Mean	SD	Mean	SD	
UPC Index in Pre Period	0.704	0.204	0.337	0.277	1.509
Share of visits with Long Term GP	0.848	0.178	0.509	0.370	1.168
Continuity of Care Index	0.553	0.249	0.206	0.245	1.406
Sequential Continuity of Care Index	0.623	0.188	0.299	0.260	1.430
Number of Different GPs	4.608	2.866	3.345	2.480	0.471
Number of Observations	21737		769445		

*Note:* SMD = Standardized Mean Difference. Based on the Years -4 to -1 before the Exit for the Treated Sample. Based on the Years 2011 to 2014 for the General Population.

### A.3 First Stage Estimates

An important question is whether the exits of main GPs actually impact continuity of care. To test this we consider the impact of the exit on several measures of continuity of care in a basic DiD setting. First, the Usual Provider of Care Index (UPCI) which is defined as the amount contacts with the most seen GP as a share of all GP contacts (Breslau and Haug, 1976)<sup>19</sup>. Second, we use the Continuity of Care Index (COCI) which considers all GP contacts and rewards having fewer providers (Bice and Boxerman, 1977). The third measure we use is the Sequential Continuity of Care Index (SECONI) (Steinwachs, 1979). This indicator not only considers the number of visits but also the order in which GPs are seen, punishing switches between GPs. These indicators are all commonly used in the literature and highly correlated, suggesting they all measure the same underlying phenomenon (Chan et al., 2021; Pollack et al., 2016). Table ?? shows that the GP exit has highly significant negative impact on continuity of care across all measures.

Table A.8: DiD Estimates on Continuity Measures

	UPCI	COCI	SECONI
<i>A. Non-Exit Practice Control</i>			
Post	-0.145*** (0.00841)	-0.148*** (0.00886)	-0.0602*** (0.00551)
Treated	-0.0270*** (0.00982)	-0.0290** (0.0118)	-0.00525 (0.00879)
DiD Estimate	-0.0978*** (0.0134)	-0.0986*** (0.0143)	-0.0534*** (0.0101)
Constant	0.730*** (0.00562)	0.582*** (0.00698)	0.628*** (0.00524)
Num Obs	163126	163320	163320
<i>B. Within Practice Control</i>			
Post	-0.170*** (0.00939)	-0.168*** (0.00939)	-0.0781*** (0.00621)
Treated	-0.0236** (0.0101)	-0.0245** (0.0122)	-0.0134 (0.00909)
DiD Estimate	-0.0731*** (0.0141)	-0.0789*** (0.0146)	-0.0355*** (0.0106)
Constant	0.727*** (0.00604)	0.577*** (0.00756)	0.637*** (0.00562)
Num Obs	99218	99402	99402

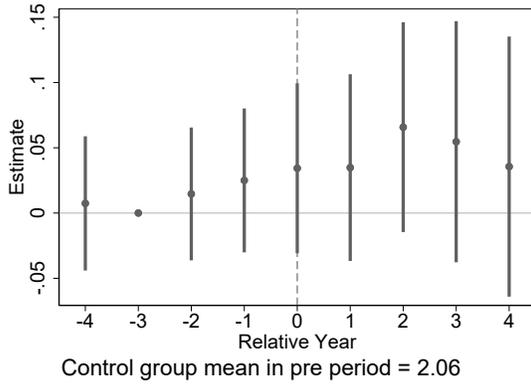
*Note:* UPCI = Usual provider of care index. COCI = Continuity of care index. SECONI = Sequential continuity of care index. The standard errors (in parentheses) are clustered on the Main GP level. \* P<0.1, \*\* P<0.05, \*\*\* P<0.01.

<sup>19</sup>Note that we exclude the UPCI if the most seen GP is unidentified

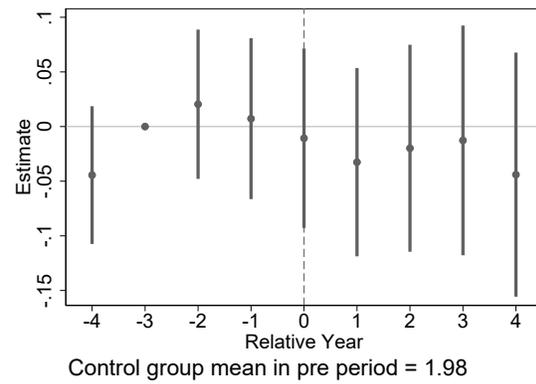
## A.4 Additional Event Study Estimates

Figure A.2: Event Study Estimates: Number of specialist visits

(a) Non-Exit Practice Control



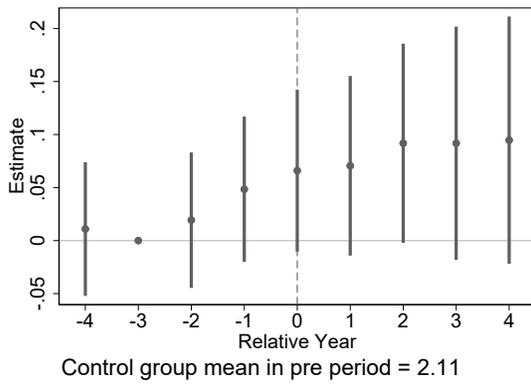
(b) Within Practice Control



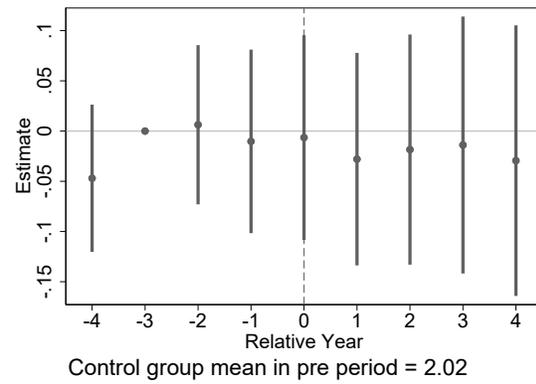
Note: Entropy weighted event study estimates.

Figure A.3: Event Study Estimates: Number of specialist visits: Patients with chronic conditions

(a) Non-Exit Practice Control



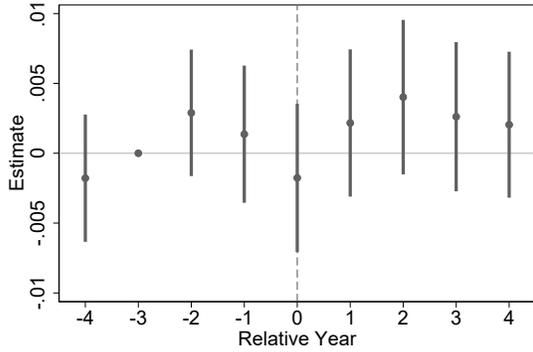
(b) Within Practice Control



Note: Entropy weighted event study estimates.

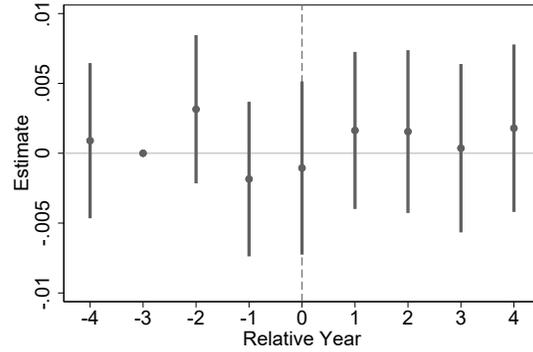
Figure A.4: Event Study Estimates: Any non-acute hospital stays

(a) Non-Exit Practice Control



Control group mean in pre period = 0.06

(b) Within Practice Control

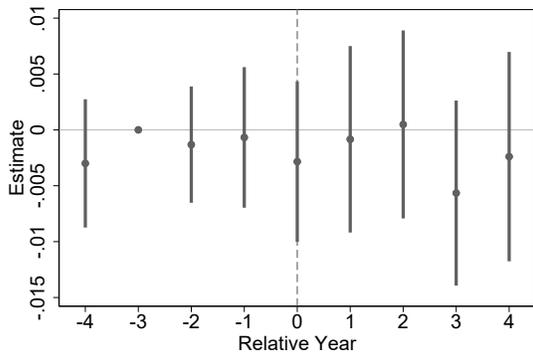


Control group mean in pre period = 0.05

Note: Entropy weighted event study estimates.

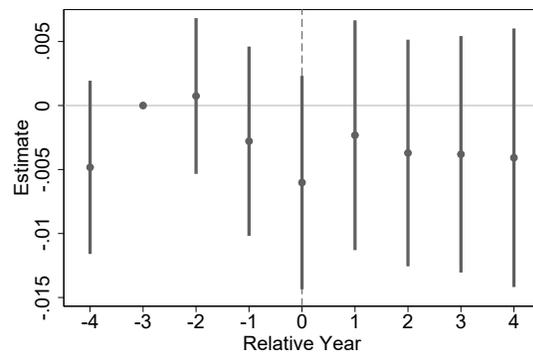
Figure A.5: Event Study Estimates: Any prescription for chronic conditions

(a) Non-Exit Practice Control



Control group mean in pre period = 0.72

(b) Within Practice Control

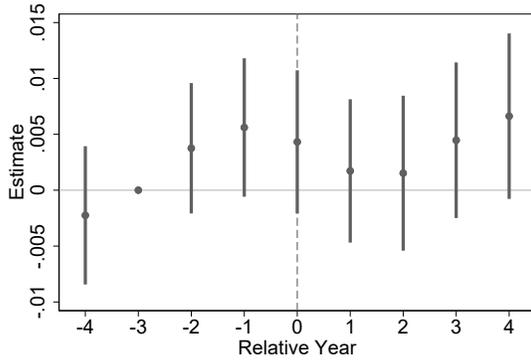


Control group mean in pre period = 0.72

Note: Entropy weighted event study estimates.

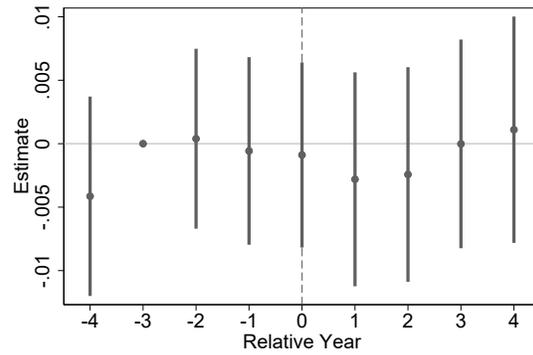
Figure A.6: Event Study Estimates: Any acute hospital stays

(a) Non-Exit Practice Control



Control group mean in pre period = 0.11

(b) Within Practice Control

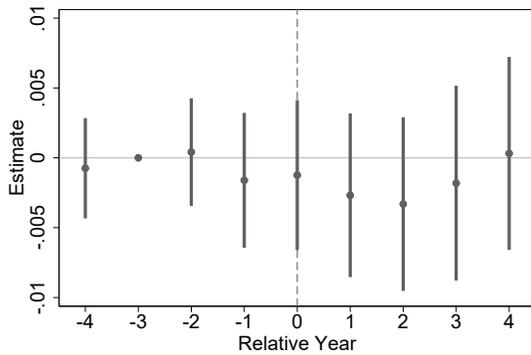


Control group mean in pre period = 0.11

Note: Entropy weighted event study estimates.

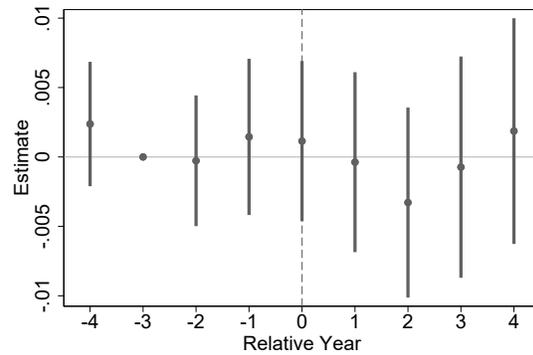
Figure A.7: Event Study Estimates: Any cancer diagnosis

(a) Non-Exit Practice Control



Control group mean in pre period = 0.07

(b) Within Practice Control



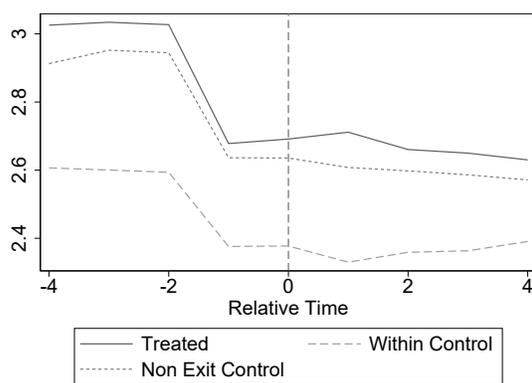
Control group mean in pre period = 0.07

Note: Entropy weighted event study estimates.

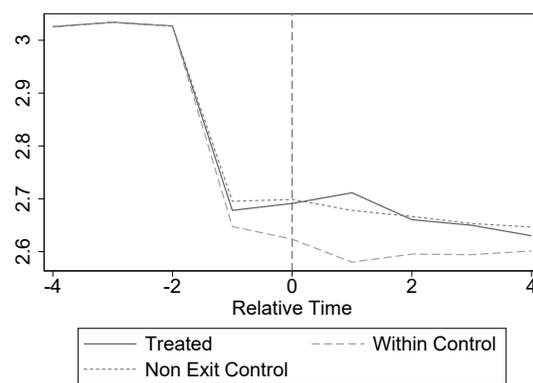
## A.5 Graphical Descriptive Results

Figure A.8: Number of GP Visits: Raw Means

(a) Before Balancing



(b) After Balancing



Note: *Before Balancing* = Means without any weights. *After Balancing* = Means using the weights derived by entropy balancing.

## B Data Appendix

### B.1 Variable Definitions

Table B.1: Definition of Chronic Conditions

<b>Diagnosis</b>	<b>ICD</b>	<b>ATC</b>
Asthma	J450, J450A, J450B, J450W, J451, J451A, J451W, J458, J459, J45-P, J469	R03AC01, R03AC02, R03AC12, R03AC13, R03AC19, R03BA, R03AK
Depression	F321, F322, F328, F329, F33-, F330, F331, F332, F333, F334, F338, F339, F32-, F320, F341, F348, F349, F380, F381, F388, F399, F39-P, F412, F40-, F400, F401, F402, F402B, F402F, F402G, F402W, F408, F409, F410, F411, F412, F413, F418, F419, F419P, F42-, F420, F421, F422, F428, F429, F431, F43.1, F32, F33, F39, F40, F41, F42	N06A
T2 Diabetes	E11, E12, E13, E14	A10
Hypertension	I10.9, I11.9, I12.0, I12.9, I13.1, I13.9, I15.0, I15.1, I15.2, I15.8, I15.9, I13-P, I10-, I15-	C02, C03, C07, C08, C09
Heart Failure	I110, I130, I132, I500, I501, I509, I50-, I42.0, I42-P	C01, C03, C07, C09, A10BJ
COPD	J440, J441, J448, J449, J449P, J44P, J44-, J44.-	R03BB, R03AL
IHD	I200, I209P, I201, I208, I209, I21-P, I210, I211, I212, I213, I214, I214A, I214B, I214W, I214X, I219, I220, I221, I228, I229, I230, I231, I232, I238, I240, I241, I248, I249, I25-P, I250, I251, I252, I255, I256, I258, I259	C01, C07, C09, C10

Note: Given are the starting digits of the respective codes.